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Edw. A. M. Hall
"to his friend
B. Mendenhall"

AN
EXPERIMENTAL ESSAY,

ON
MERCURY.

BEING AN

INAUGURAL DISSERTATION,

SUBMITTED TO

THE EXAMINATION

OF THE

REV. JOHN ANDREWS, D. D. PROVOST, (Pro Tem.)

THE

TRUSTEES AND MEDICAL PROFESSORS

OF THE

UNIVERSITY OF PENNSYLVANIA,

On the 3d. day of June, 1805.

FOR

THE DEGREE OF DOCTOR OF MEDICINE.

BY JOHN DOUGLASS, 1803
OF VIRGINIA.

HONORARY MEMBER OF THE PHILADELPHIA MEDICAL SOCIETY.

New truths are seldom discovered, but at the expence of old errors.

RUSH.

PHILADELPHIA:

PRINTED FOR THE AUTHOR, BY JOHN H. OSWALD.

.....
1805.

TO
JAMES WOODHOUSE, M. D.
PROFESSOR OF CHEMISTRY,
IN THE
UNIVERSITY OF PENNSYLVANIA.

DOCTOR ALEXANDER SANDERSON,
ELISHA CULLEN DICK, M. B.

AND
SAMUEL JONSTON CRAMER, M. D.
THIS ESSAY,

IS INSCRIBED
AS A MARK OF ESTEEM
AND
GRATITUDE,

BY THEIR MUCH OBLIGED AND

RESPECTFUL FRIEND,

THE AUTHOR.

THE UNIVERSITY OF CHICAGO

THE DIVISION OF THE PHYSICAL SCIENCES

DEPARTMENT OF CHEMISTRY

CHICAGO, ILLINOIS

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INTRODUCTION.

THE *modus operandi* of mercury, on the human body, has long been a matter of controversy among physicians. A question so interesting could not fail of exciting great attention, and accordingly, it has been the subject of much speculation and theoretical dispute. All who have written on this subject, have agreed in considering mercury a stimulant; but with respect to its peculiar manner of acting, there have been various opinions. This is well calculated to shew how fond physicians have been, of rejecting the true, but too little trodden path to truth, which is experiment, and of wandering in the unbounded regions of speculation and hypothesis. And we will not be surprised at this, when we consider that it is easy to

form a theory, and one which without examination will appear plausible ; but that to adduce facts to support this theory, is much more difficult.

The difficulty and laboriousness of experimenting, appears to have been a great cause of preventing physicians from pursuing this mode of investigating truth ; and hence, the same phenomena have been attributed to various, and even opposite causes. To prove the truth of this remark, it would be necessary only to take a view of the theories which have been advanced with respect to the operation of the gases upon the human body, in producing disease. Dr. Girtanner for instance, supposes scurvy to be produced by an excess of oxygen, while Dr. Beddōes imagines it owing to a deficiency.

Equally unfounded with these hypotheses, is the opinion of those authors, that mercury owes its activity to oxygen alone. This theory, though perhaps not generally adopted, appears to have advocates, which

will not appear strange, when we consider the celebrity of its authors, and the easy method which it suggests of explaining the operation of a medicine so little understood; both of which have generally too much influence in leading to the adoption of an opinion, without enquiring into its justness.

It is this theory that I shall particularly notice, in the following essay, and to which my experiments are principally directed. I have chosen it as the subject of my inaugural dissertation, from a conviction that the experiments of the late Mr. George Lee, the only person who has written in opposition to this doctrine, are not accurate, and consequently not sufficient to establish the opinion he has advocated. I have repeated the experiments of this gentleman, with some variation, and have made some additional ones, which I thought necessary to arrive certainly at the truth, and to put the question in dispute beyond doubt. I do not wish it to be understood by this, that I pretend to have ascertained the manner in which the several preparations of mercury, made

by trituration, are fitted to act on the system; for this, if it be discoverable, would require more experiments to ascertain it, than the time allowed me to prepare this essay, would admit of my making. I have merely reduced to a certainty what has not been hitherto proved, and by very few believed, that the mercury in the blue pill is not oxydated, but only in a state of extreme division.

EXPERIMENTAL ESSAY,

ON

MERCURY.

Dr. GIRTANNER appears to have founded his theory of the *modus operandi* of mercury principally upon the well known fact, that mercury in its crude state, when taken into the stomach, or applied externally to the human body, exerts no influence upon the system. He has also laid considerable stress upon the assertion, made by some, that this metal, after having proved active in the human body, has passed through the pores of the skin, revived.

As crude mercury is inert with respect to the human body, we are naturally led to attribute the activity of its several preparations to a chemical change; but it will not, I am persuaded warrant the theory which Dr. Girtanner delivered.

With respect to mercury's being thrown out of the body, in a revived state, after hav-

ing produced its usual effects, admitting it to be a fact, I do not consider it as tending greatly to prove the doctrine, which it is made use of to support. For it is well known that many articles of the *materia medica*, produce their effects without suffering any considerable loss of weight. I think then, from analogy, we are authorised to conclude that a mercurial salt, or oxyde may act on the system, and afterwards be decomposed, and the mercury discharged from the body. And that the fact, if it be one, will by no means admit of the inference which Dr. Girtanner has made, that “the oxyde of mercury, in passing through the human body, parts with its oxygen, and it is to this oxygen alone, which remains combined with the system, that the effect produced by oxydated mercury is owing.” This hypothesis, however, was conceived under the influence of a belief, that mercury, in all its preparations, was in a state of combination with oxygen.

To enter into a minute detail of the arguments which have been brought forward in favor of, and against this doctrine, would be

unnecessary, as it is by experiment alone that the question can be decided. It may not however be amiss to take a cursory view of some of them; and I shall only notice some of those adduced in favour of it by Dr. Glover, in a paper on this subject, published in the sixth volume of the Medical Repository of New-York, in answer to the observations of Mr. Lee, before mentioned.

Dr. Glover has tried to prove, first, "That it is not essential to the theory of oxygen, that an oxyde of mercury, when taken into the system, should be revived, to produce its usual effects": 2ndly, That certain effects are not to be attributed equally to all the constituent principles of a compound body; and lastly, That it is to oxygen alone that the several preparations of mercury owe their activity."

The first of these positions I shall not consider; as whether it be true or otherwise, it cannot affect the point under our consideration.

To shew that certain effects are not to

be attributed equally to all the constituent principles of a compound body, the atmosphere is mentioned as an example, one of whose constituent principles only, has any share in some of its effects ; of which, combustion is mentioned. This I do not consider as an argument greatly favouring the doctrine which the gentleman advocates. For it is not certain that the component parts of the atmosphere are in a state of chemical combination ; but on the contrary, arguments might be brought forward to render it at least probable that they are not ; and if this be the case, it is easy to understand how the oxygen of the atmosphere supports flame, while the azote remains passive.

But if the constituent parts of the atmosphere were in a state of chemical union, this would not go to shew that oxygen is the only active principle in a mercurial oxyde, or salt.

Before taking any further notice of this gentleman's arguments, I shall relate an experiment of his, which he supposes proves that the mercury in the blue pill, is in the

state of an oxyde. This experiment was made by rubbing the pill in a mortar, and washing it in warm water, by which the mercury was not revived, but appeared, as he supposed, in the form of an oxyde. It is alledged that mercury will not unite with water, and as all the other component parts of the blue pill are soluble in this fluid, an inference is made, that if the mercury in this preparation were only mechanically divided, it should be revived by the above mentioned process. This however is certainly erroneous ; for I shall presently shew that mercury is miscible with water, and that the blue pill cannot be deprived of all its vegetable substance, by washing.

It is objected to the doctrine of Girtanner and Beddoes, that oxygen, when taken into the lungs, does not salivate. This Dr. Glover endeavours to do away, by saying, that “ Vital air diluted, and in the state of atmospherical air, is a stimulus to which the lungs are accustomed; its effect therefore, will not be expected to be such as, when applied in a highly concentrated state, in the

form of corrosive sublimate, to the stomach, an organ by no means accustomed to its action. In the one instance, it is a natural and accustomed stimulus ; in the other an artificial and preternatural one."

In answer to this I would observe, that other metallic calces and salts, do not salivate, or if they ever do, it is very rarely. The rust of iron, for example, may be taken in large quantity, and continued for a great length of time without producing this effect whereas a quantity of mercury containing much less oxygen than that combined with the iron, will uniformly induce a ptyalism. Beside this, we every day take into the stomach with our food, perhaps more oxygen than is contained in a quantity of mercury sufficient to salivate. Upon the whole, this doctrine would seem to rest upon a very slender foundation, even if it were true, as has been supposed, that mercury in all its preparations, was in a state of combination with oxygen ; but it will appear still more absurd when this opinion is proved to be erroneous.

Before relating my own experiments, it

will be proper to take a general view of those made by Mr. Lee, which I shall next do, and endeavour to point out their inaccuracies.

This gentleman's first experiment was made by triturating mercury, rhubarb, and conserve of roses together, in a marble mortar, until the mercury disappeared. These ingredients were weighed before the trituration was commenced, and after it was finished; the mass, on being weighed, was found to have suffered some loss, which was attributed to evaporation, and from which it was inferred that the mercury had absorbed no oxygen. This experiment was repeated with the same result. Mercury, conserve of roses, and flour, were next rubbed together for two hours, and the loss by weight, which took place, was fifteen grains. By way of comparison, the same quantities of flour, and conserve of roses were triturated together without mercury for the same length of time; in which there was a diminution in weight of thirteen grains.

These experiments are all liable to the same objections, which are, first, The im-

practicability of getting the mass all out of the mortar, must have made it impossible to make an accurate calculation of the loss which took place, during the trituration; for what adhered to the mortar when the mercury was triturated, was much heavier than that which was left in it, in the comparative experiment: secondly, The impossibility of repeating the experiment under exactly the same circumstances*; 3dly, It is well known, that in triturating mercury in a marble mortar, part of the mercury will insinuate itself into the pores of the mortar; and lastly, The impossibility of determining the weight of oxygen.

Mercury and hogslard were next rubbed together; first in the same manner with the above experiments, and afterwards in a certain quantity of atmospheric air, confined over water, by means of a common apothecary's bottle with the bottom cut off, and a bladder tied over the mouth of it; through which a pestle was passed. This bottle was placed over a glass mortar, fixed to the shelf

* This Mr. Lee himself mentions as the cause of the difference of loss in the experiments.

of a pneumatic tub. A portion of the air was then drawn out of the bottle by a syphon, and the height to which the water arose in the bottle was marked on the outside of it. After the trituration, the water appeared to be at the same height as before.

No accurate conclusion can be drawn from these experiments; for the following reasons; first, The heat of the hand placed on the bottle, and the heat produced by trituration, must have considerably rarified the air in it ; and, secondly, Air may have been given out from the water.

Having taken a view of Mr. Lee's experiments, I shall now proceed to relate my own,* of which those made by trituration, were performed in the same manner with this gentleman's last experiment, in a certain quantity of atmospheric air.

In order to do this, I tied a piece of bladder, doubled, over and round the neck of a

* It may not be improper to mention that most of these experiments, especially the principal ones, were made in the presence of Professor Woodhouse, who was satisfied with their accuracy.

glass jar, with a perforated top. In the part of the bladder which covered the mouth of the jar, an incision was made, through which a long pestle was passed, and the bladder tied firmly to it; leaving a sufficient length of bladder to allow of the free motion of the pestle. A small glass mortar was then placed in a plate of water of about the depth of 2 inches, and into the mortar were put two drachms of mercury, and about the same quantity of flour and water, made into a paste. The jar was then placed over the mortar and the trituration commenced, which was continued for four hours.

The purity of the air confined in the jar was now tested, by passing one measure of it up over water, into the eudiometer of Fontaine, and afterwards a measure of nitrous air, procured from copper, a solution of nitre, and sulphuric acid, when there was an absorption of ninety-five degrees. A measure of common atmospheric air was then thrown up into the eudiometer, and tested in the same manner, and the absorption was the same. Nitrous air however not being an accurate test of the purity of gas, I had recourse to phosphorus.

A small portion of phosphorus being placed over water, in the eudiometer, containing a measure of the air of the jar, and left to stand for forty-eight hours, produced an absorption of twenty-one degrees ; which shews that the air was unaltered.

The mass in the mortar had become blue, but on inspecting it closely, I found that the globules of mercury had not entirely disappeared. With a view, therefore, of completely extinguishing the mercury, I repeated this experiment, with the difference of making use of rhubarb instead of flour.

EXPERIMENT II.

Two drachms of mercury, and twenty grains of rhubarb, with a sufficient quantity of honey to make the mass of such a consistence as to be easily rubbed, were triturated together for three hours, at the end of which time, the mercury was completely extinguished. The air was tested, and the absorption was twenty-one degrees.

EXPERIMENT III.

Two drachms of mercury, and three of gum arabic were triturated together for four

hours. At the end of this time, the air was tested, and the result was the same as in the former experiments.

EXPERIMENT IV.

Having proved by the foregoing experiments, that the mercury in the blue pill is not oxydated by the oxygen of the atmosphere, I next wished to ascertain whether it received oxygen from any other source. For this purpose, I procured from the Pennsylvania Hospital, a quantity of the blue pill.* Hot water was poured upon this until all the soluble parts were extracted. The residue, which was of a black colour, was then exposed to the sun, until it became perfectly dry, when a considerable quantity of the mercury was revived. The mass was next placed on an iron support, under a bell glass, over water, in hydrogen gas, obtained from the filings of malleable iron, and diluted sulphuric acid. The focus of an eleven and an half inch lens was then thrown upon it, and continued for fifteen

* Some of the mass of which this was a part, had, I was informed, salivated several patients in that institution.

minutes. During this time, the air in the bell glass was so much expanded, as to cause the water to flow over the edge of the plate in which it stood, and part of the air made its escape. This was produced by the heat of the lens, communicated to the support, and the inflammable air dissolving a portion of the charcoal of the vegetable substance, with which the mercury had been triturated.

The whole was now removed from the influence of the lens, and set aside to cool; after which the water in the bell glass, was observed to be a little lower than at the beginning of the experiment, in consequence of the waste which took place. One measure of the air contained in the glass being thrown up into the eudiometer, over lime water, gave about one per cent carbonic acid gas: the white precipitate of carbonat of lime was *not* ~~and~~ perceptible, and the fixed air which gave rise to it, was previously contained in the hydrogen gas, as appeared by testing it.

On a lighted taper being applied to the inflammable air, after the very small quantity.

of carbonic acid gas had been separated from it, it exploded in the same manner as hydrogen gas, holding a small quantity of charcoal in solution.

This experiment proves, beyond a doubt, that there was no oxygen given out from the mass ; for if this had been the case, the oxygen of the oxyde of mercury would have united to the hydrogen gas and formed water, in consequence of which, that in the bell glass would have risen.

EXPERIMENT V.

Mercury and hogslard were triturated together for four hours ; at the end of this time, the air in which they were rubbed was tested, and found not to be diminished in purity.

EXPERIMENT VI.

For the purpose of separating the mercury from mercurial ointment, I put a portion of this into a phial, which was exposed for a considerable time to a degree of heat sufficient to keep the ointment in a fluid state. It was then suffered to cool ; and on examination, the mercury was found to be nearly

all revived; the lard having in a great degree, resumed its natural colour.

EXPERIMENT VII.

I next wished to ascertain whether mercury would be acted upon by the gastric juice; for which purpose I obtained a quantity of this fluid from the stomach of a dog that had fasted forty-eight hours. A portion of this liquor and some mercury were put together into a phial, which was placed in the axilla of a person, and kept in that situation ten hours. At the end of this time no change was observable in the mercury, except that of simple division, which took place to a very great degree upon agitating the phial. This experiment, however, affords no accurate conclusion, on account of a variety of circumstances.

EXPERIMENT VIII.

Six eight-ounce phials, marked 1, 2, 3, 4, 5, 6, containing the following ingredients, were fixed to the spokes of the wheel of an electrical machine, by means of which, the
included

included ingredients were agitated for six hours.*

- No. 1. Four ounces of pure mercury, and two ounces of water, in atmospheric air.
2. The same quantity of mercury and water, in oxygen gas, which devoured nearly four measures of nitrous air, and gave, by the test of phosphorus, five per cent. azotic gas.
3. Mercury alone, in atmospheric air.
4. Mercury, iron tacks, and water, in atmospheric air.
5. Mercury and vinegar, in atmospheric air.
6. Mercury and water.

Having taken the phials off the wheel, they exhibited the following appearances.

The water in Nos. 1 and 2, appeared of a black colour, but soon became transparent ;

* This experiment, which, though not intimately connected with the subject of this essay, I thought, might with propriety be introduced in this place, was suggested by a similar one which Dr. Sanders made. He included one pound of mercury in an iron box, with a quantity of iron nails, and a small portion of water. This box he fixed to the wheel of a carriage, and during a journey of four hundred miles, there were four ounces of a black powder formed, which the Dr. alledges was a true oxyde of mercury.

the colouring matter having subsided. This matter was collected by filtering the water, and exposed to a gentle heat, when it afforded nothing but running mercury. The gases were unchanged.

No. 3. Not the least alteration had taken place either in the mercury or the gas.

No. 4. The air was unaltered. The water appeared of a black colour, arising from particles of iron detached from the tacks, as appeared by collecting and dissolving them in the sulphuric acid diluted with water, when upon the addition of the alcohol of galls, a black colour was produced, and by adding the prussiate of potash to the solution, it yielded a copious precipitate of the prussiate of iron, or prussian blue.

No. 5. None of the contents had suffered any alteration.

No. 6. The water was slightly discoloured.

From this experiment it is clear, that the black powder produced by the agitation of mercury in water, is nothing more than a mixture of these substances, and not a calx of mercury as has been by chemists imagined. Chaptal supposes it to be an oxyde, and

that the oxygen contained in it, is derived from the water* in which the mercury is agitated. This however is proved to be erroneous, by the experiments of Dr. Priestley, who found that the power of water to produce this powder, is not lessened in the smallest degree by repeatedly agitating mercury in it ; which would unquestionably be the case, if the powder were owing to a combination of the oxygen contained in the water, with the mercury.

It would be unnecessary to make any remarks on the foregoing experiments, as the true conclusions from them must be obvious. They prove beyond a doubt, as was before observed, that mercury, in at least one of its preparations, namely, the blue pill, suffers no chemical change before being taken into the body. The question then naturally occurs, to what does the mercury in this preparation owe its activity? This, though there have been many conjectural explanations given of it, must be acknowledged still to remain in obscurity. Every opinion on the subject appears to be liable to objections.

* What is meant above is the oxygen diffused through water.

Some have supposed mercury, in the several preparations made by trituration, to be rendered active by a minute division of its globules, thus fitting it for absorption. But this, admitting that the mercury is absorbed, does not account for its activity. For if it undergo no change, except that of simple division ; if it suffer no chemical alteration, after being taken into the system, it must still be in the state of crude mercury, which, when taken into the stomach, though it is not absorbed, is applied to the alimentary canal, without affecting it. Another opinion on this subject is, that mercury is acted upon by the phosphoric acid, supposed to be always present in the gastric juice. This opinion, however, is certainly hypothetical, and not very plausible. For it is uncertain whether the phosphoric acid exist in the gastric juice, and if it be contained in this fluid, the quantity must be so very small, as not to act on mercury, in any considerable degree.*

The most probable conjecture which has been offered, to explain this phenomenon,

* Mr. Lee mentions that he could not detect this acid in the gastric juice of dogs, by means of blue vegetables.

appears to be, that mercury is dissolved by the gastric juice. This opinion is strengthened by a fact which is related, of copper having been dissolved in the stomach. It is also alledged by some respectable practitioners of medicine, that iron-filings are equally active with the oxyde of this metal, which, if it be true, would seem to shew, that iron undergoes a solution in the body. And, from analogy with this, it appears reasonable to infer that mercury suffers the same change.

It may readily be perceived why this metal in the form of blue pill, should be dissolved in the stomach, while in its running state, it suffers apparently no change. Trituration, by dividing minutely its globules, exposes a larger surface to the action of the gastric juice. And beside this, it must be evident, that running mercury passes out of the stomach very soon after being swallowed; whereas by being intimately blended with other substances, that is prevented, and time thereby allowed for the gastric juice to act upon it.

This however does not account for the activity of mercurial ointment, the metal in which it has been rendered probable, is in the same state with that in the blue pill. If this be the case, and if mercurial ointment be taken into the system, of which there ought to be I think no doubt, it might be supposed that this metal is dissolved in the circulating mass. But this is mere conjecture. That mercury however, in the above mentioned preparations, undergoes in the body a change by which it is fitted to act on the system, is I think certain, but where or in what manner this is effected, remains yet to be determined.

FINIS.

